

Oct. 5, 1932
Wed. night

Dear Barb:

Stadler has just sent off your paper. He has been busy on his Congress paper. I made quite a lot of suggestions, which, of course, I want you to consider only as such. Not being able to talk them over, I may have given them quite a different emphasis, ^{or slant} from what you had intended.

not much of any news, or so it seems. -

I guess I haven't written you though since I worked on the Δ which is closely linked with α . The break is on the short arm of #9 and out beyond the knob of #5. At Δ - , out of 106 figures, 58 had 10 Π , 40 had a chain of Π , and 8 were rings. Apparently most of the breaks to form chains on ^{or in} at the end of the pr chromosome; although of course the other one breaks very frequently (from the evidence on the number of "pairs").

In prophase, there is a lot of sliding. I have found the cross coming off on the short arm side adjacent to the insertion region, bringing the pr knob at the cross. It slides on out the short arm of #9 (no figures found ^{slid} in the other direction), out to where the pr arm of the cross consists only of a few chromosomes and on out to where these ends don't seem to reach - or they simply failed to synapse. I got a few photographs but they aren't particularly good, except for my
I am enclosing some sum ones !!!

own records. There isn't a great deal of ~~synapsis~~ synapsis. I wonder, if ends tend to synapse first, all one would need to give all this sliding would be for ~~a block of~~ the different chromosomes to start in this process at different times, at least not at the same instant. Or it conceivably might be different rates. It might be a matter of chance, depending on conditions, which one ^{or ones} started first - and this might vary at each division. One would expect ^{some} sliding in all translocations, but the ones with unequal pieces interchanged should tend to show the most. I will have to make my counts for crossing-over in corn, but last year's evidence indicated no decrease.

I worked a little on my 4₂₋₃, but figures weren't very good - possibly the weather has been too cold. We are expecting a frost tonight, but may miss it. My corn still needs a week or so to bring out the tassels.

Some more counts on #8 mut 1 with chocolate, out of 250 plants, about $\frac{1}{2}$ were chocolate. Doesn't look like it is in #8. Does Emerson have any dope? I am writing him.

Well, when are you heading west? - Will you be able to stop off here?

Oh yes, Cal. Tech. hasn't lost a scope - must have been at Mich. I had a ^{long} letter from Andy, in which he seemed to have very few complaints.

He even offered to give me board and room out there if I get where I want to move. I am afraid it wouldn't be too good out there, ~~unless~~ I could manage ~~to~~ to keep my spirits up - for several reasons.

I wouldn't mind stopping off at Riverside - but Leslie is going to send me ~~some~~ ^{some of} seeds of ^{the} ~~tomato~~ ^{tomato} mutants.

Have you been playing tennis? I haven't been out, but have played golf a couple of times. I saw the 4 Mary Proc. in Horse Feather - rather good in spots, also a little raw.

What do you have that is new? Did you hear from Lammeets. He wrote that there is some quadrivalent formation along with that Carmine-coral business.

Well, Barb, guess I will sign off and write two other letters.

~~With~~ Sincerely,
Charlie.

Oh yes, I see Brad's paper on Trosents is out. Did I tell you I looked at 3 Taddler's 7, and didn't find any insertion regions which didn't correspond. Also, there didn't seem to be as many knobs in this material. - this ^{latter} could vary.

(I had to reopen this!)